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MISSOURI BOTANICAL GARDEN
GEORGE ENGELMANN PAPERS

Germantown Nurseries,
THOMAS MEEHAN,
Proprietor,
Office, 5547 Germantown Avenue,
PHILADELPHIA.

Rec'd Aug 24 ans same day

Germantown, Jul. 21 1872

My dear Dr. Engelmann

I do not feel tied about your accidental overlooking of the letter. I do worse sometimes. There is a total absence of Zoi cones here this year. When I received your letter about this I wrote to a friend in the "Frazer" requesting that he report "no cones" and will look to it next year. Your fertilization will be attended to. I will try several dwarfs to find if any show aided fertilization but from more which probably avoided. There were a large proportion of a very small species which I did not know flitting about the flowers but these were always in and about the base of the perianth and I don't think operated directly upon the apex of the carpels.

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in Black and not colored.
Now we may make out with
dried specimens, but it strikes
me the difficulties of coloring
except from living specimens
will be so great that they
must either abandon the
coloring plan, or else the
~~diplomatic~~ idea.
There has been
no definite action taken.
We have only issued a
few copies of the prospectus
I sent you, and these
only as I sent you —
& I have only sent to
Gray, Torrey, and yourself.
Gray writes as you do — Torrey
I have not heard from, but
I see by the papers he is away
west. When the matter
is in final shape I will
let you know.

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2

Germantown, 187

There will be no text in the "Plants" – It is intended to illustrate the work of Prof. Blackman in his own agency will chiefly be to see that the subjects are properly identified before drawing. Those who have so distinguished himself in Conducting, learning and acute botanist and will see to the accuracy of the drawings. I offered as you do for a long time the "Systematic" project but it was urged that manifestness not care to have the whole set but some, like to show the of the members, and thus that urge the systematic project. This is now as far as it does and might do if they are to be

4/

I could not get time to go away this summer as I wanted to go to Dubuque to see what they know about the formation of wood. My own observations lead me to believe, that wood is not formed by the "descending sap", but by the germination of cells one from another; and that these cells push simultaneously over the whole tree in little colonies, coalescing and forming a homogeneous layer of wood several days in some cases taking a week after germination (of the ~~[illegible]~~ last year cells) has commenced. ~~In the~~ There are several ~~of these~~ "broods" of cells during the season, and in the final brood of the year the matter is stored with which

Germantown, 187

to make another layer of wood next year. I have found by careful experiments that the stumps of trees do increase in girth (the head being cut away) ~~or~~ after cutting away but only one year unless some sprouts are left to grow, when they will increase for several. But they will increase over year without any sprouts or ~~[illegible]~~ fertilizer from the stump.

I do not know that these observations are entirely new, but I do not think the general public understand to a matter this way. Gerard is very

6

feeble. almost the whole academy is away somewhere this year. But we are going on with the new building. The cellar walls are in hand. Gabb came in from San Domingo last night — but "had no time to get any plants" which is too bad. They are trying to turn Hooker out of New York, as they did Asa Gray from Washington. but I think Hooker will have better luck.

Very truly yours

Thomas Meehan

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Rec'd Sept 24

Germantown, Sep. 2 187[illegible]

Dear Dr Engelmann

The land on which my blooming yuccas were, I sold this spring for building purposes – Having thousands of plants on my new grounds, I did not move the few old ones this weeks ago, the new landlord made a brush heap and burned it right on the yucca bed. But the capsules are bored precisely as yours are, and have been so long past as I can remember – thirty years back. I have found yuccas vary so much from seed that I have no idea you can determine the species from the leaf alone. I fear yours. Y. filamentosa is both Y. filamentosa. I have tried to distinguish some by the narrowing of the petals, but even

I feel like you a little dubious about the ability of the artists in the Illustrated Flora – my only ground of confidence is that Engelmann is eminent as conchologist, and has been for years engaged in these illustrations and descriptions, and will know well the worth of looking well to the characteristics – especially as he is now paying close attention to Botany. I do not think however the intention is to go into minutiæ so much ~~as~~ to aid critical Botany as to aid general students who have the descriptions of Gray and Chapman before them, an idea of the plants referred to.

Nurseries,
MEEHAN,
oprietor,
Office, 5547 Germantown Avenue,
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MISSOURI BOTANICAL GARDEN
GEORGE ENGELMANN PAPERS

Germantown, 187

though these narrow and broad petals should remain fixed for a hundred years in a direct succession of annual generations I should fear to regard them as species. I have not as much courage as my friend Cope who tells me if I were to "evolve" a couple more molars than common ~~the~~ he would make a new genus out of me. — I have Yucca glaucosa, but it throws up a flower stalk quite before Spring, and gets destroyed before the buds open. We also have one called in the Gardens Y. recurva pendula & very popular in ornamental gardening but though I have old transplants it has never bloomed.

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I observe by the public papers that my theory of sex came up at Dubuque, and that the various speakers assented to the view, so much that it is regarded now as the settled faith of the association. As I have not been able to satisfy our good friend Dr. Gray, so far as I judge by his correspondence of the strength of this position, I suppose surely he must have been absent from the session while the discussion was going on. ~~I should very much like to have~~ I wish I had been there. I could now furnish the most abundant evidence, that a little more or less of nutrition in the early foetal conditions of life (plants or animals, if the terms be allowed to the former) makes all the difference in the sex.

Very truly Thurber

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Rec'd Sept 7 '75
Ans Sept 29

Germantown, Sep 4th 1872

Dear Dr. Engelmann

I release you from some proofs of plates we propose to issue. I fear you understand us to be engaged in a more learned work than we aspire to. The intention is to furnish students, and the "public in general", with some companion ~~in the~~ to the descriptions as given in Gray and ~~Darby~~ Chapman. It is not for critical Botanists, but for the public at large. Tell me if you think in black this way will do? I am afraid the cost and difficulties of coloring every plant of Gray and Chapman will be insurmountable. These

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cuts were made chiefly
to ascertain exactly the
cost and abilities of
the artists and rich-
ness we have decided
positively to go on with
the work. The prospectus
sent to a few friends is
also intended as a feeler
of our way

Very truly yours

Geo. Meeker

Rec'd Dec 6th

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GEORGE ENGELMANN PAPERS

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Dec 4th 1873

My Dear Dr Engelmann,

Asa Porter writes to me (some minutes ago, but I have overlooked it till now) for some account of the Yucca thoberiana I found in Colorado. The small one I did not find at all this year. He and Coulter purpose to have and daughter next report on flora of Colorado similar to Watson's Robertson King's rept. and they will probably have it ready for the press in 15 or 20 days. As he is now making use of my collections in the report, I thought he would like if you could send me a note which I could give to him about the Yucca thoberiana, as having given you the specimens they have got in hand. I have sent to him and he as A. Gundersonii Dammeri abundantly, the were most of our trade last year.

When in Colorado in 1871 the Yucca angustifolia was everywhere seeding abundantly

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this year, though he searched every where, I did not find one capsule. where has your Proserpina gone?

When living in the Isle of Wight in England 30 years ago, there was a Yucca gloriosa, which if time has not magnified the appearance in my memory, was lovely, perfectly upright, and with a stem I should think at least 12 inches in diameter, and still growing rapidly. It had a branching head, the branches standing I should think 6 feet from the ground. It flowered every year, but I never saw no seed vessels – though often meeting Yuccas at that time I never saw a seed vessel that I remember.

Hoping you are bearing lots of interest and that we shall hear about it this or the time

Sincerely yours

Thomas Nuttall

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Rec'd June 15
an — 16

June 13 1874

Dear Dr. Engelmann

For years I have wondered why Calycanthus floridus rarely seeds in the North. Of millions of flowers we may get one perfect pod. ~~I thought~~ In the Tennessee it fruits in enormous quantities, and the true seed market has been for years flooded with seeds from that region. I thought our lower altitudes were too hot for it; but in Mr. Shaw's garden it seeds in great abundance. Then I thought that perhaps (as in nurseries we keep up the race from one plant) we had a barren individual to work on; but of late years numerous seedlings have blossomed, and all have the same barren peculiarity. I came to the conclusion since your Yucca observations, some insect might have a hand in it. I find however as the result of my spring examination, that each flower covers its own pistils abundantly with pollen. I am reduced now to this question:— is cross fertilization necessary? and what is the agency which accomplishes

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of variation — no more than if every tree were cast in the same mould.

I enclose a leaf of an English and American Beech. There does indeed seem to be much in the vaguer distinction about the nerves running into the serrures in one case, and into the apex of the serratures in the other. Of course there is no difference but terminating in the serrures & does not express it.

Yours truly

M J Berkeley

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Rec'd June 15th

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R. Pursh

787

it? As the plant seeds near you, perhaps you or Riley can find out. Some of its near allies, Magnolias, (I regard Calycanthaceæ and Magnoliaceæ to present such wide structural differences fundamentally because in the former there has been an extraordinary arrest of growth in the central axis to form the flower, while in the latter it has rather been accelerated) have similar peculiarities — of dimorphism and one many in a thousand perfects here, — and in Magnolia Fraseri, conspicua, and very often M. acuminata it is the same.

There is another odd peculiarity in Calycanthus (shared in by Chimonanthus) that whereas in most genera of few species there are few variations, the Calycanthus from seeds vary very remarkably in foliage. In Stappleia Mitella, and some others, I can find not the slightest trace

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Rec'd July 10th
Ans — 14

July 8 1871

Dear Dr. Engelmann

Did you know that in Acer Stratum each alternate pair of leaves are smaller and narrower? I never saw it till to day. I have about a thousand trees growing, and 10 have this character. It is so plain that I think some one ought to have seen it, but I have never heard of it. I send you a few leaves to day.

Your oak is doing well. As it grows (I have a Q. palustris of the same age near it to compare) I see that it is an imbricaria in the texture of its leaf. Besides I have a bed of Rev. Richards of two year old Q. palustris, but I cannot find one leaf but is thin as this species always is. I thought the first year of your seedling that it might be Q. palustris. I have to alter that opinion now. I should say that it is imbricaria, that has planted off to lobe its leaves. I suppose all this can occur without any hybridization

with other things. You know things will start off in this way. The Maternal that naturalization facit saltum is not true. Our rhododendrons have taken to having leaves this year 5-7 lobed instead of three lobed as of olden time. On young trees one half about are this way — and on old trees nearly all. I suppose they have only taken to this this year, as I think I should have noticed it if it had been before.

I have a young filaverde in which years ago started to bloom ten days before the others. Young plants retain the same character. The "struggle for insect fertilization" could not have induced this trick, for I see there are none seeding on these plants. The others which have abided their time are getting well. But I suppose plants can go crazy as well as men

I look up this spring two year old seedlings of Yucca angustifolia, and of Y. filamentosa (remembering your letter of the 6th last) and find no difference in the manner of sending out the secondary shoots. They were both downwards. In regard to your grape question I do not think cutting off the supply of nutrition (stunting) has any influence on sex. I have seen Ambrosia artemisiaefolia but one inch ~~and~~ high, with male and female flowers, just opening in full grown plants; and bearing seed just as well. It is only when something in the cells (impaired vitality I call it) which interferes with the cells ability to avail itself fully of the nutrition at command, I can I take it that the laws of sex are affected. I have noted in the proceedings of my meeting an instance of this kind in the Chestnut. I believe grapes change sex sometimes in hot houses. Muscat of Alexandria is known as a bad setter; some

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form. At Martinus' all I have had no chance to examine of late years, but we used to suppose it was caused by abortion of the stamens. I think in Hort. Acc. Nat. Scientiae there is a specimen purely male, but that may be an individual characteristic of some plant, and not a character in that individual as I think is almost irrelevantly said.

In regard to the losing of leaves I used to think the leaf had to take the form its nerves said perforce, — and I was rather surprized when in one of your letters you suggested the possibility of nerves and veins being a subsequent and not a formal act of formation. I am now satisfied that is the correct view, that what we might term leaf blade is first formed, and that nerves, ribs, petioles and stem are all leaf blade differentiated. Dr. Gray does not think so however. I sent him some specimens last

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week pointing out as I thought this inevitable conclusion; but he only sends me in return some specimens of another plant and asks what they mean? and truly I think they mean just as much as our specimens did!

Our botanist? — Charles Smith seems not to have recovered from the lethargy which followed the terrible strain of the Reading R.R. Presidency. He comes to our meetings sometimes, and I think with an increasing interest. Aubrey H. Smith, Cattell's assistant, is warm in interest as ever. Mr. Burk continues my right hand man at the Academy (though suffering badly just now from the death of his only daughter, about 25 years of age) a patient steady worker, the Academy's herbarium is indebted to him more than anyone is aware. We have been greatly favored by good friends the past few years with specimens, and several thousands of new ones have

been added. All of these have been poisoned by burning, their ~~spaces~~ labels allotted to the specimens, and the initiatory steps taken for distribution ~~[illegible]~~ to their proper places. In the distribution I have had good help from a young man, whom I expect will prove our best botanist in time – Dr. Henry Hoffmann. We were pretty well advanced up, but a few nice packages from Dr. Gray has set us on our feet again for a four months job. We are now making a catalogue of all our species in which Parker and Redfield are taking a hand. Hooper is off to Europe again!

Very truly as ever yr

M. Curtis

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GEORGE ENGELMANN PAPERS

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Rec'd Nov 1/10

Oct 14 1871

Dear Dr. Engelmann

Of all things I was glad to get the Drosera conductata. So many have promised to get them for me; but never did. I suppose the branches you sent me were of Picea (Abies) concolor. I soaked it in water for a day, and then made a graft of it, and believe it will grow. Numerous accidentally put as you sent me, I found in 1871 on a foot hill to the Eastward of Pikes Peak mixed with J. virginiana. Dr. Gray doubted this, but when he was here last year, together we compared it with a "Whipple" Spec. and he decided he could see no difference, and finally said "no one but Dr Engelmann could tell the difference". I am much interested in what you told me of the Sonoran. I have been looking for our fall one which is very scarce. I have found but two specimens, one of which I enclose. It is closed. I have always supposed it to be the P. Andersonii. But never yet critically examined it. 92 agrees with your extreme smaller observation: so the few I have seen the anthers are united, and the pistil forces itself through the anthers,

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but

this may have been an accident. I suppose at this late season there is no time to mature its seeds before frost, and I cannot conceive why so late? Why so closed a flower? You say the stigma is like my catalpa. I suppose you refer only to the ~~cleft~~ cleft before protruding, and expanded after. Not the structure — I find no resemblance in this. In other respects, the expansion is inside and the stigmatic surface is covered by its overfolden.

An hasty train morning, ever truly yours Asa Gray

I wish I could get the seeds of the Abies concolor? without destroying the cone — for the academy.

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Rec'd Nov 17th

Nov. 14 1874

Dr Geo Engelmann
My dear Sir

Just a hurried line in acknowledgement of yours of 10th. The acorns of Q. undulata are safe. The other two are down, and I have no doubt will also come out right. My grafts of A. concolor are green yet, and I believe will come out right. Q. occidentalis was too dry-dead!

I had but one specimen of that Witicum. The reports published have scarcely done me justice; but the official ones will. Dr. J. G. Hunt is Chairman of the Microscopical Section, and a capital botanist. It was my own motion to refer to that Committee, because I did not care to cut it to pieces myself. The Committee report the thing is a fraud. "Neatly inserted, and fixed with gum Tragacanth". I can only say that nothing of this could be detected by strong lenses — and everything appeared just where a morphologist would expect to find a growth.

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pleasure, that a very full meeting was interested that night at any rate, and in spite of the ridicule on me now, I shall continue to do what I can to help them along.

I shall write again about some of the matters down —

as ever truly yours
Thomas Meehan

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787

It would perhaps have been better had I not introduced the matter publicly till the specimen had been cut to pieces. But there has been much complaint by our citizens who are asked to subscribe to our new building that the meetings are dull and uninteresting, — that "nobody says anything", — but that "they come and go and nothing new is learned". On this account and with the thanks of the President and others, I always try to help along, and not suspecting fraud, I thought the handling around of this to the members before cutting it up — with a few remarks bearing on the subject — would interest. Which it did very much. And although I am very much laughed at now by those who know no better now I did then but know a great deal more, I still remember with

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MISSOURI BOTANICAL GARDEN
GEORGE ENGELMANN PAPERS

Rec March 22,
Ans May 23

Germantown Pa 1875

My Dear Dr. Engelmann

I owe you at least a couple of letters. I had a chance to spend a few weeks in Mississippi, and worked very hard to get all my work ahead, "piling up" letters not immediately pressing, and now the good friend who was to "pelt" me has met with family afflictions, and I suppose forgotten me. So here I am.

Without your letters before me, I thought I must drop you a line to say why they are unanswered. I gave the one about the "Coral or Pea Shell" to Dr Leidy, who promised to answer it, and I suppose has. The Iuncus undulata are growing well, I am very glad to say. You had a few in two separate papers, and I sowed them separately. One pot full came up the plants and leaves two weeks ago, and are now 9 lines 3 inches high, and leaves expanded, — the others are but just sending up the sprout from the ground, — both pots treated

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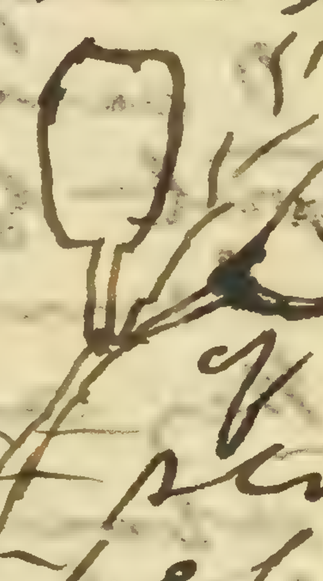
both exactly alike. I suppose you collected the nuts from different forms? The Juniperus monosperma is also just sprouting up above the soil — no begins yet even of a radicle in the D. simulata. If my recollection of sprouting acorns does not deceive me, the petioles (?) of the cotyledons in D. undulata are longer than usual. They are one third the length of the blade (?) I may however be wrong in my recollection of others, but I was never struck so much by the length before.



One of the grafts of Abies concolor is good. It has not grown any yet, but I think it safe. I enclose a few seeds of Juniperus ~~sc~~ californica var. utahensis. Nancy refers to in March am. Naturalist, in case you may not have seen it. Excuse this scrawl for a letter very truly Your Wm. Mecham

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Rec'd Jun 3d

At Leyden & P. virginiana

Germantown May 30 1875

MISSOURI BOTANICAL GARDEN
GEORGE ENGELMANN PAPERS

Dear Dr Engelmann

I have been sick for some two months, chiefly inflammatory rheumatism — and, now, getting better again, I was planning a ride with my wife for this Sunday afternoon when your welcome letter came. So I decided to go to a little wood of Pinus virginiana that I had seen some 15 years ago. It was of about 20 acres equally divided with red cedar, and was then about 15 years old. How they came there I do not know. The undergrowth grass, moss, Mitchella and so on, with a few Blackberries. There is no other lot of Pinus virginiana nearer to me than New Jersey. — Well it was a pitiable sight to day. Nearly the whole outside of the Pine killed by some borer. Most of them had been cut away as they died, — but a few of the dead and dying were still standing. I send you specimens to day. I could get no fresh mature cones. The old ones under the trees crumbled like dirt in being touched. I found two or three young trees that had not yet borne, with male and female flowers, and from these I send. The cones in this wood (and I believe they are always so) appear sessile when mature, but the young flowers show there is a short one, and this I believe is common to all pines. The cones are pendant, though sometimes on vigorous branches, several will be round the stem, when the extra ones are erect. I have had a great deal of experience in Pinus virginiana, and contributed a paper on ~~them~~ its "variations" to the Proceedings of the Academy U.S. 1848, 304 while a boy on 6th day. The spines vary in length but I believe are always recurved. The tree is spreading in its growth and when mature flat topped. The variations noticed by me are chiefly in the form of the cones. I have never found any that I thought specifically different to virginiana except once, when botanizing with Canby in Delaware. There was something in the look of the tree besides the narrow ovate cones, but on getting it home I concluded it was but virginiana. I send you a specimen of it. I gathered it at Moxlentown Pond. Del. 1860.

my nursery education has unfitted me for great faith in "new species" at every new form that appears, — nor does "natural hybridization" satisfy me. I feel that our best botanists have no idea of the great breadth covered by natural variations. With our hundreds of thousands of seedlings, and a keen eye we see it. I have two plants from your seed of Quercus Nigrifolia. One has very thick, dark green leaves, and at first glance I thought it was Oreodaphne californica. No one I am sure would suppose it to be of the same species as its brother plant along side of it. I am happy to say that I raised one of your 2 Sinuata. It was kind enough to have a double kernel, and so I have two plants from the one acorn, as Quercus alba or often does (and robur). In the two lots of Q. undulata, besides one taking up over to Terminalia, the leaves are a little less lobed. It is however too soon to say much of their fistulaceous characters. The second or third year tells the story. Your intricaria or whatever it is, is growing finely. As soon as the leaves get a little thicker I will send you some. The first year it was like Q. palustris. It is not now like any palustris I ever saw and yet entire like intricaria. Still I have seen so many curious forms independent of "hybridization" in trees, that I would not like to agree with you about its hybrid origin, any further than to say it may be, but "unlikely". I hope to have a plant from it for Mr. Shaw this spring. We use Quercus robur as a stock for all oaks. The grafts grew together and I throve at all, was right. But

though the union seems perfect not one pushed out, and all are dead now. ~~I have~~ I will try it on 2 palustris this season. — The cotyledons of 2. umbellata separated, because I took off the exocarp, before the germ pushed. I do this always when I want a good strong healthy plant of a rare oak. The light then makes a good stock of chlorophyll in the cotyledons, which then act in a measure as leaves. It is quite likely it would not have entirely burst if my knife had not aided it. Nature is not worth a cent in a mother against art. Your Abies concolor graft is still as fresh as when it was put on the stock — A. balsamea but it stands quite still. The terminal bud shows no signs of growing, though its foster parent — the Balsam — has made its seasons growth since. I will try and get grafts from Mr. Brandegee. I greatly prefer grafts to seeds of these fine evergreen coniferæ. It is a "terrible" thing to raise them from seeds. It takes "years" to make a pretty a few inches high.

I am amazed at the rate they make species out of our coniferæ in England. They know how all other genera vary and it is only once in a while that some one takes up a genus, perhaps Erophila or Ribes, and makes hundreds of species of them. They never seem to think that there may be varieties among coniferæ — the forms must be best species. Every twist or turn of the leaf and the most trifling differences are

specific characters. You ought to know much better than I about Abies concolor, – but if the bracts are to be the chief distinctive I proved fear to depend on them. In some coniferous species they vary scandalously. (See my paper on the bracts in Larix) and perhaps Abies grandis may be in this category.

I have lots of things I would like to say to you, and to ask you, but must now. In particular I want a name for that peculiar wave like growth of some plants. In Pinus for instance, some species will have the female flower terminating the young growth. I think all do this in a measure, but some make another growth, and often a third, and thus the cone seems lateral (Pinus funegus) again in Phytolacca we have a strong shoot from an axil bud, – the next is weaker, the third very weak, the fourth wanting, but in its place a raceme of flowers and this gradation of vigor is always in this regular order. The same in Polygonaceae, though sometimes in Boraginaceae the three nodes become almost fasciled in flower. In that curious genus Calycera, which I have just now in flower, these vigor waves are biserial. In endeavoring to account for a similar phenomenon in Acer Pensylvanicum I used the term "arresting force" in relation to these waves of decreasing vigor, to Dr Gray but he criticizes it, and I fancy somewhat justly, but offers no better one.

As ever truly yours Ned. Lee Meehan

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P.S. I suppose Dr. Gray looks on "an arrest of growth" merely in a passive sense, - and his knowledge of language is so clear that I hardly dare to express any difference with him; yet I can hardly conceive of a living plant otherwise than as an actor, and that "arrest" as applied to plant functions, may be an active verb as well as a substantive. and that "a arresting force" may be used to represent the power which changes the current of growth into various channels?

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GEORGE ENGELMANN PAPERS

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PHILADELPHIA.

Recd Oct 13

Ans Nov 24

Oct. 11 1875

Dear Dr. Engelmann

I am very much obliged by the acorns. The tendency to pedunculation would have suggested to me that this was diversus discolor. In this however I think Q. macrocarpa plays some part. We have here no undoubted seedling of Q. macrocarpa but we can hardly tell from Q. discolor. Indeed Q. macrocarpa is quite a study. Last month I saw trees near Chicago the leaves of which were exactly like Q. obtusiloba, but there was not that peculiarity of long branches along a few main stems that gives valley oaks that peculiar, indescribable character. Indeed I think the diversus & macrocarpa rather very small fruit.

I go after the in America!

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United, and then died.
I have some this year on
palustris, and they look
all right. I shall send
them some seeds to Mr.
Drew, and will send
one, for you to see from.
I found a purely
Cordate form of Nopalus
monilifera in Santa
Barbara Cyon Ck on
Sierra Nevada exp. – but as
I sent the specimen to Dr Gray
I will say no more about
it now. I mail with this
a copy of my Detroit paper.
It is not however correct,
as I read it. The correct was
in a recent Gardeners Chronicle.
But this will give you an
idea. Very truly yours
Thos. Meehan
The Colorado cactus are doing well –
but the draft of A. Concolor, finally died.

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Rec'd Dec 15
Ans — 17

Oct. 13 1878

Dear Dr. Engelmann

I have just returned from a trip to Mississippi, where I had a nice time among the water-oaks, and so forth. I am quite satisfied now that De Candolle is right in making Quercus heterophylla Ait., but a variety of Q. aquatica. We find "heterophylla" rather commoner now.

I write at present particularly about Quercus ~~aquatica~~. In your copy of De Candolle's Prodromus at the Academy, you have noted that Q. "aquatica" moved five ways to the Q. Balloonana. Would you be good enough to tell us?

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have been to day ex-
amining the plants in
the "Bot. Ex. to Oregon" —
but allowing for the
great variation in the
Cases and Covers of
Plants, it looks very
different from our
Colorado R. or its tributaries
I have a healthy
small plant of it from
a graft I brought from
Gray's Peak.
When I get more
to see about finding

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PHILADELPHIA.

Rec'd Dec 15 75

787

Your condolence at Genl Grier's death I have letter from Penny at the same time — and from your two letters together I conclude that it was only the light cured forms which I gathered in Canon Wiegelmann that you thought to be A. concolor, and that the other — the death Cone Pine was true A. grandis. I have had [illegible] recently with

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George Engelmann Papers

Mr. Brandegee, and he thinks I am under a mistake in this. He believes that you have not all that we have called A. gracilis in Colorado is A. concolor, and that the true A. gracilis does not exist there. This is his opinion at any rate. There is a fine cone of A. macrocarpa from Dr. Vasey. Please let me hear about P. Balfouriana as I am anxious to know what you regard is as the same as Panilata

J. M.

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MISSOURI BOTANICAL GARDEN
GEORGE ENGELMANN PAPERS

Rec'd Jan 14th
Ans May 7th

Jany 12 1876

Dear Dr Engelmann

Your card to hand. I must seem a little ~~remiss~~ in my correspondence. The fact is I have been away off and on so much this year that I am getting confused. I see it has been altogether four months out of 12! and I am thinking of going into Miss. again next month. Your letter on Wellichan's oaks I want to look over very carefully again soon. Somehow I ~~hesitate~~ to accept much in the "hybrid" line. Only think of Kerrier and twenty eight hybrid Rosaceae in the genus! And then the Canadian American of hybrid origin! Is there anything in our experience that will warrant us in believing that nature proposes herself to have perpetual accident?

However in Miss. we will talk I note through having had

a hint from Murray in which he seemed to think his evidence for "Balfourana" conclusive. Amongst other things ~~I told him~~ I was under the impression that you had told me when I saw you that you had examined his cones at Edinburgh, and I wrote to you to the same of this point. I have written to him and told him you saw ~~his~~ the seedling plant at Edinburgh, and I also said that you saw the specimens at Kew. I believe this is the substance of all I told him of your experience and I said it was only for his own private use as to the "reasons why Am. Botanists differed with him".

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I have hitherto given my own views, derived from my own studies in the lines of variations which various ~~[illegible]~~ types of conference letter, and which lead me to regard the two as one. I should have perhaps sent them your letter for his private reading & on to let him see your views, but I felt there was much intended for my eye alone.

My growing plant of P. amicilla ~~[illegible]~~ from a soft stock at Germantown, always has two leaves only in a pseudo-whorl. I should expect to see three, and perhaps five in branches where the growth force is weak. I know this in Phinnsylvanicum and others.

Of course I shall

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say no more publicly about
P. Lewis' letter till I know
more. I was tempted
to say what I did be-
cause Mr. Murray's
remarks about the
timber valuable for
Railroad purposes, and
other errors about
other timber pines were
so flagrant, that I felt
nettled that the English
people should think that
was the good end of
all that was known
about them.

Hoping to get a chance
to write again soon, and
hoping you have recovered
from your little accident, I
am as ever truly yours
Thos. Meehan

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GEORGE ENGELMANN PAPERS

Rec'd May 15th
Ans same day

May 12 1876

My dear Dr. Engelmann

Though with piles of letters unanswered, and heaps of business unattended to, I do not like to reply to your very welcome letter. At least to promise that I will reply. For the present, I will say that a few days ago I sent a plant of your hybrid oak to Mr Thaxter and asked him to put it where it will grow and you see it. I enclose some young growth of it. Also of your hybrid linnaeta at least I have it marked simply "linnaeta". I forgot that you thought it a hybrid, — or did you send me a typical linnaeta and a hybrid as well? If so I have lost the one. The "hybrid" albus (Chinese) mis-name are doing well. I forget me of your only in my collection of

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hardy ligneous plants on the Centennial grounds (800 species and well over 1000 varieties) I never tried to distinguish the few others. My Pinus (the one I have talked about) is contorta! The contorta grafts I sent home in 1873 all died, and I did not know I sent any home in 1871. Lying sick on my back in Druid Park, I got a companion to visit for me since Aristotelias which I pointed out to him from a distance, and which I sent home. I thought they had all died. In 1873 I cut them myself and, I have thought till now, this was one of my own cuttings, and I feel sure of it.

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There was another "stunted" strip on my grounds near it in the same line. Examining with my foreman into the strip since your letter, I found one of my 1877 grafts did grow, and that in a contorta which my friend had in some way mixed with the real varietas. Fortunately the "stunted" strip is one of my own cutting of R. varietas. I found a fascicle of it. My foreman had placed the two together, supposing from the "label" it was the same, while I was left to suppose they were both off my own. I am amazed in the error, yet glad in the fact that it results in my having both where I thought I had but one

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But you say "avis tata on well as flexilis" name entire "Cranes"? Well I enclose you flexilis. Surely I am not in a blunder about this also, – but you see the Genera are more decided than on the contrary I send with it.

I must close in haste, – but hope to enjoy another pen chat with you soon.

Very truly Your
Thos. Meehan

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GEORGE ENGELMANN PAPERS

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Rec'd July 3d — Ans 29th
Ans verbally & Lan

Germantown July 1st 1876

MISSOURI BOTANICAL GARDEN
GEORGE ENGELMANN PAPERS

Dear Dr. Engelman

It gave me great pleasure to receive Mr. Letterman's yesterday, and find in his hands your oak notes. I had a party from the East, and West, and South so happened that I could only entertain him in common with them. Charles E. Smith still takes great interest in Botany — as much as he can in anything. Putting the great Rail road ruined his brain. He would highly esteem your paper I know. We have made a Botanical Section to our Academy, and I am sorry to say, with me as Vice Director, which is practically the head of it. It ought to have been Canby, who has more time and more fitness than I have. Had I been present, as I intended, and as I should have been but for waiting an hour in an appointment with Henry Ward Beecher which he did not keep, I should have decidedly refused it.

The oak paper. — I am pleased that you have thought to make a character of the bark and wood. I have been struck with this for some time. Quercus undulata I since always recognize in all its varieties by its bark. In examining various woods at the Centennial I was struck by it more than ever, and in the "Botany of Centennial" which I am contributing to the New York Independent, and of which the first 3 papers are now in their hands, you will see how I have made use of it. Looking over my notes, and comparing with your tract, I see what may interest you "judging by bark and wood I think Q. lobata may be the same as Q. undulata" "Q. texana Buckley in all probability Q. rubra"!

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"2. Durandii Buckley, distinct from all other oaks in bark and wood."

As for 2. heterophylla I feel that you have been led astray by those in Bartrams and Marshalls, and probably by those in Lindley, which were most likely distributed by Col. Carr from these Bartram garden trees. I am sure they were never seedlings from the original ~~Bar~~ heterophylla tree of the Col. Carr. I am sorry to say after his death, was I have found utterly unreliable as to such facts as these. That (the) trees are 2. Phellos — lobed forms nothing more. I have had good chance to see 2. aquatica by the thousand last fall and winter in Miss. and La. They have leaves long petiolate to sessile — lobed and entire — linear and broadly wedge shaped — lobes naked and bristly setaceous. It is possible that by some mimicry 2. aquatica and 2. heterophylla are distinct, but I have not seen it ~~since~~ yet. Canby mis took me. I told him you said "specimens of 2. aquatica received from Wilmington" proved to be but Phellos!

When you come — you must see me here. I can show you ~~[illegible]~~ in quantity of most of our native oaks, and you can judge of their several lines of variation better that way than any other. The palustris — imbricaria, approaches palustris in foliage more every day. This accords with your law of blending. — a very good point against me. I see it more exemplified in Ranunculaceae, Cruciferae and others.

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Acc'd July 3rd 76.

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GEORGE ENGELMANN PAPERS

If I had Yucca brevifolia, I think I moved down there for fruit. I could not get any of Y. arborescens here. The Pronuba were lazy, and neglected their work. From one to five of them were in each flower from the opening of the first flower to the last, but no seed. I took the flowers on stamen, and tipped the "mouth of the crater" with the pollen, and every flower so tipped fruited. What is the food of the Pronuba in cross fertilization? I intend to ask this question of Buffalo, if I can get

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time to work up the paper, for two days a week a centennial prize, — 4 newspapers, — a large business, and so forth, drives me closely. I have given much time to cross fertilization the past year. Strange that nearly all the best illustrations of cross fertilization, self fertilize — even Iris seeds pistils as well under a fine gauze bag, as out. Only Baptisia australis, of all I have tried does not do as well under protection as outside. As ever truly yours

Ind. Freeman

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GEORGE ENGELMANN PAPERS

Rec'd Washington Sept 16
Ans Nov 1 Dec 7

Eff. 2 1876

Dear Dr. Engelmann

I am just from Buffalo, where nothing was wanting but you, Dr. Gray, and a few other friends to make our happiness complete. I am very glad to get the "hybrid" Verbena, because it is a subject on which I am seeking light. Mind I do not say there are no hybrids in nature — that some oaks and this Verbena are not hybrids — but that my experience among so many forms of living plants and their variations makes more proof desirable to me than perhaps is necessary to satisfy other minds.

I really think this is as likely to be a variation by the power of some innate law of development as by intercrossing. It has certainly an intermediate facies — but the little peculiarities of

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THOMAS MEEHAN

Office, Upper Church & Che

Sept 14 EAR PHILADE

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Rec'd Washington Sept 14

787

V. Stricta seem well preserved. It is decidedly heavier, not fluccose as I believe it is in urticifolia. Besides here is the structural difficulty. The mouth of Verbena is so densely clothed by hair that an insect drawing out its proboscis would most likely have the pollen cleared off it, not to say the extra chance by the "second wipe" on entering another flower. When I have found in Verbena that the anthers are ready with their pollen about the time that the corolla expands, and the entrance of the insect would rather knock the pollen down on its own pistil — self fertilizing by insect aid! Are not these facts against the idea of hybridity?

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Will you ever note
the stigma of Verbena
stricta? In fact our
common garden Verbena
it is but in this
all your (memory) like for
horses foot. I do not
remember how it is in
V. urticaefolia, but it is
worth working into in view of
this hybridity question.

Hoping to see you here
soon I am very truly your
Thomas Meehan

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Rec'd Nov 1st
Ans Nov 7th

INTERNATIONAL EXHIBITION.

1876.

MISSOURI BOTANICAL GARDEN
GEORGE ENGELMANN PAPERS

United States Centennial Commission.

BUREAUS OF ADMINISTRATION.

AGRICULTURE.

Philadelphia Oct 28 1876
Germantown

Dear Dr. Engelmann, — I hope soon to be through with my heavy centennial work, and then to work up more closely matters that have been interesting me this summer. I wish you had called to see my collection, — I really believe you would have been more interested, and have found better material for study at my place than anything you have seen in the North. I have not only one or two — but hundreds and often thousands of one thing and of all ages and sizes, but I am not much given to boasting. I try to make my friends feel that they are heartily welcome, not only to see me and my place, but also to anything I have but I am not much "on the press" if I think they prefer to see other places. I think it strange sometimes that no one comes. I thought certainly I should have had you or Gray, or Sargent here this Summer. The two last have been here to Phila — several times this Summer, and have been to lots of little places where they could see nothing much, but not to me. Canby has never been to see my place, that I remember. I have tried hard to get Thurber, but have never succeeded, — till I almost fancy I have Botanical small pox or some other fearful malady!

Well, I want to hunt up more about Lycoris helianthella. I have here

an amateur horticulturist who has connections with Maryland, and who is an acute observer. He brought me from Maryland a leaf of an oak to name. It was dried up, but enough to show it to be our old friend — petiole and all. I can see little difference any where from aquatica except in the length of the petiole. After all is this of much account? I enclose you leaves from two different parents of Quercus robur. I am almost sure the trees are from seed from one tree. I am not quite sure. One of my hands brought me the seeds some years ago from a distant property the owner of which says he has but one English oak. But I know how error may arise, and he may have two and not know it. On the other hand I have hundreds of trees from this lot of trees, and the petioles in different trees vary so, that I may say I have all sorts of lengths. I think they must be all from one ~~tree~~ parent tree — the two I send — yet how different the lengths of petiole! But you will say why showed all the aquaticas of New Jersey, Del, and Maryland have long petioles; and those of the South have short ones? Well if you had seen any trees of other species from different localities, they would answer the question. It makes no difference what the species is from different localities they are not exactly the same. In a few specimens the differences do not show, — but when you see hundreds of trees together

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INTERNATIONAL EXHIBITION.

1876.

MISSOURI BOTANICAL GARDEN
GEORGE ENGELMANN PAPERS

United States Centennial Commission.

BUREAUS OF ADMINISTRATION.

AGRICULTURE.

Philadelphia 187

It is a very different thing. You write that what I sent Sargent is not E. aquaticum. I do not know now, what I sent — that is the block I sent from, — but I have aquaticum from different places that do not look like the and yet what else are they? one lot is of a beautiful dark shining green leaves, almost as thick and shining as holly leaves. When we see the whole block a little piece off, you would almost think they were hollies and not oaks. Now I have another block which look like oaks, — but if you pick off a few leaves of these ~~and~~ two blocks and look at them, you can hardly detect one from the other. Still I am "bothered" a little with the E. heterophylla. If I did not know so much of these oaky tricks, I should have no hesitation in tying up at once with you. It is a species. Since I wrote to you at Cambridge I find that under some conditions, the Norway spruce has laterally compressed leaves, — and it has also often twisted leaves, so much so that the under and whiter surface is uppermost. I was walking through my grounds one foggy morning recently, and looking a block where there were some

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of four year old Spruces, and also quite some thousand of three year old ones; and on the latter the fog seemed to have stuck fast, while the others had no cloud over them. It was on the three years I found the twisted leaves. It made the upper surface flat, and the fog rested on it better than on the rounder leaves, and gave the plants the misty look. I think we cannot make anything from Conifers by leaves alone.

I have a few fruits of what we call Carya sulcata for you, but am wanting to find some Microcarpa for you. Our men have sent in some for it already this year, but they are but very small forms of C. glabra. I suppose you know that some forms of glabra split into four pieces almost by the wind in drying, while others have an indehiscent epicarp.

I do not know how other botanists feel, but with me I learn so much now every year that I wonder that I knew so little last, and it makes me feel that what seems so clear and conclusive now, may be all wrong next! I am more inclined now than I was when with you to look on Joshua Hoopes Celtis Crassifolia as a sexual form merely. I find certainly that the deeper the lobes of a white oak, the more fertile it is, and when a Liriodendron has nearly entire leaves it is nearly barren.

Very truly Thos. Meehan

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THOMAS MEEHAN, Proprietor,

Office, Upper Church & Chew sts., Germantown,

NEAR PHILADELPHIA.

Rec Jan 15th
Ans April 25

MISSOURI BOTANICAL GARDEN
GEORGE ENGELMANN PAPERS

Jan 11 1877

Dear Dr. Engelmann

Your excellent letter I must read over two or three times, to day I will only talk of the hickories. I thought I knew about these, but with your and Lettermans material I do not know what to think. I have picked out from Lettermans his smallest fruits, and from my largest tomentosas the largest ones. They are in bags marked one 1. and 2. Can you tell which are yours?

I also send a few of our Carya glabra. They are about the average of dehiscence. They often not the slightest sign of division, at other times so much that they can be opened with good fingers.

Very truly yours
Thomas Meehan

over

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What we should
call Carya tomentosa
[Jenina are sharper
a little on the edges
than these.

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GEORGE ENGELMANN PAPERS

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THOMAS MEEHAN, Proprietor,

Office, Upper Church & Chew sts., Germantown,

NEAR PHILADELPHIA.

Rec^d May 10
Ans safely
RC

May 7 1877

Dear Dr. Engelmann

On account of a brief trip I shall make to Maryland four weeks from to day, I am so busy getting necessary literary work ahead that I am ashamed to seem so slow with much valued correspondence, but I cannot let your oak go longer. To day I send some expanded and unexpanded buds. It is a strange customer! Monopolus as you know I doubt its hybrid origin (chiefly from seeing its much natural variation) I would not say it is not. I am standing on this question with a balance pole in my hands!

I believe I told you that I would start to illustrate in chromo lithography all the wild plants of the United States. I think it will be in my friends forwardness to commence this fall. By some new discoveries in

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the work, I hope to be able to get them out good and cheap, and (what is more important to a man of 5/ respectably. I think it quite likely we may get out 75 a year with three pages of text for about ten dollars a year; but this is not definitely decided. Although we rule the botanically accurate of course, it will be addressed to "lovers of wild flowers" chiefly; — my object being to recruit botanical ranks from mere flower lovers. I have a large number of photos with text ready, but want to have many more so that I may go into the [illegible]

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Stephens hands while I am away. Leading botanical friends discourage me from the work as they have so much. that I have undertaken in the past. but I really think my past has been useful, and though I expect to "make some errors" and not do "just right" here and there, I hope to be more useful than if I did nothing, so I am bound to go right on!

This is a very poor apology for a letter, into one or two of yours still on my "waiting" desk, — but you see how it is. I hope to be back the 1st week in August.

Ever truly yours

Thos Meehan

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